

trations accompany the description; some of these are original, and others taken from the French, and none the worse for that. Most of them are well executed, but intimate friends might possibly find some fault with the likenesses of living men of science. Of course it is an easy as it would be a thankless task to point out sins of omission, and perhaps also of commission, in a book like the one under notice. Such works must not be looked upon with the eye of microscopic criticism. If the general direction which the author takes is the right one, if he does not make his task easy by glossing over all the points of difficulty, but puts his case clearly and fairly forward, he may well be excused if he omits matters which one or other of his readers may deem necessary. These conditions Mr. Routledge, as it seems to us, has satisfactorily fulfilled. We can therefore cordially recommend this "Popular History of Science," believing that it will exert a healthy influence on all who read it, and may be a powerful means of spreading the love of science amongst the rising generation. H. E. R.

Class-Book of Elementary Mechanics, adapted to the Requirements of the New Code. Part I. Matter. By Wm. Hewitt, B.Sc., Science Demonstrator for the Liverpool School Board. (London: George Philip and Son, 1880.)

MR. HEWITT has probably had a better chance than any other teacher of knowing by experience the working of the meagre science-subjects of the new educational code. The defects of that code, and particularly of its directions as to the subject of mechanics, are very great; nevertheless the little book which Mr. Hewitt has produced shows how, in spite of the disadvantageous system under which he works, a really good teacher will succeed in working up the subject for his pupils. We have seldom met with a really elementary book which at once combined to so great a degree simplicity of language, accuracy of description, and sound science. Mr. Hewitt states as his experience that the main difficulty has hitherto been to get the children to express in anything like precise language the ideas suggested to their minds by the simple experiments shown them. He therefore intended this little work to serve as a lesson-book to be read by the pupils in the intervals between the experimental lessons. This first part covers the ground prescribed by Schedule IV. for the first stage. A second part, dealing with "Force," is in preparation, and will embrace the subjects of the second and third stages. We hope Mr. Hewitt's second part will prove as satisfactory as is his first instalment. His aims are limited, indeed, by the requirements of the Code, but within those narrow limits his success is great.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Sir Wyville Thomson and Natural Selection

I HAVE at least great reason to be thankful that my stupidity has not prevented me from thoroughly enjoying the teachings of Mr. Darwin and Mr. Wallace, which I confess to having regarded as chiefly masterly and charming "studies in variation," for the last twenty years.

The title of the epoch-marking book which came of age last month was, however, "The Origin of Species by Means of Natural Selection." Mr. Darwin, as I am well aware, has put forward this mode of the origin of species as a part only of a hypothesis which is universally looked upon as a supreme effort of genius.

It seemed to me, rightly or wrongly, that the fauna of the enormous area forming the abyssal region existed under conditions which held out the hope that it might throw some light upon a question which appears to underlie the whole matter, and which is still unanswered. Are physiological species the result of the gradual modification of pre-existing species by natural selection, or by any similar process; or are they due to the action of a law as yet utterly unknown, by which the long chain of organisms rolls off in a series of definite links?

I fear I scarcely follow Mr. Darwin's illustration. If one were to pay his first visit to a breeder's, and be shown a flock of Leicesters, never having seen or heard of a sheep before, he would see nothing but a flock of sheep, and would certainly, without justly incurring the contumely of the breeder, be entitled to set them down merely as a group of animals of the same species, that is to say, animals fertile with one another and producing fertile progeny. He would judge so from their common resemblance, and without previous observation or information I do not see how he could know more about them. But give him an opportunity of comparing the results of breeding throughout a long period of time, or of observing the process of breeding over half the world, which comes to much the same thing; the breeder might then have cause to rail if he had not picked up the stages of the process.

The close examination of the newer tertiaries and the careful analysis of the fauna of the deep sea seem to me fairly to represent these two methods; both of these promise to yield a mass of information in regard to the course of evolution, but as to the mode of the origin of species both seem as yet equally silent.

I will ask you in a week or two for space for a short paper on "The Abyssal Fauna in Relation to the Origin of Species."

C. WYVILLE THOMSON

Rapidity of Growth in Corals

THROUGH the kindness of M. Parrayon, captain of the French man-of-war *Dayot*, I have received a large coral of the fungia tribe, which was yesterday found attached to the bottom of his ship as the copper was being cleaned by native divers. The following is the history of the occurrence. The *Dayot* entered the tropical waters of the South Pacific about seven months ago, coming directly from the coast of Chile. She visited some of the islands, but made no long stay in harbour until she reached Mauga Reva (Gambia Islands), where she remained for two months in the still waters of a coral basin. On entering the basin she touched the reef slightly, and without sustaining any damage. From Mauga Reva she sailed to Tahiti, where she now lies.

Several specimens of living coral were found attached to the copper sheathing, that which I have received being the largest. It is discoidal in shape, with its upper and under surfaces respectively convex and concave, and near the centre of the under surface there is a scar, where the pedicle by which it was attached to the copper sheathing, was broken through. The disk measures 9 inches in diameter, and the weight of the specimen (now half dry) is 2 lb. 14 oz. On examining the under surface another disk $3\frac{1}{2}$ inches in diameter is seen partly imbedded in the more recent coral growth. Of this old disk about one-sixth part is dead and uncovered by new coral, and is stained of a deep blue colour from contact with the copper, while the outline of the rest of this old disk is plainly discernible, although partially covered in by plates of new coral.

My impression is that on touching the reef at Mauga Reva nine weeks ago a young fungia was jammed against the copper, became attached, and subsequently grew to its present dimensions.

The case affords an interesting illustration of the rapidity of the growth of coral in these waters. R. W. COPPINGER
Tahiti, August 13

Geological Climates

SINCE contributing the chapter in the history of the Coniferæ upon which Prof. Haughton remarks, I have seen *Aracaria Cunninghamii* growing in gardens round Funchal, and my belief in the specific identity of the Bournemouth Eocene plant is further strengthened; yet still, as only foliage is known in the fossil, I should hardly be prepared to contest upon that alone a question of climate, however minute the resemblance. But even with the most undisputed identity there are so many possibilities

of error in arguing from a single species, that little importance should be attached to conclusions drawn from it.

Assuming them however to be specifically identical, as I myself believe them to be, and to have required precisely the same temperature, I think Prof. Haughton's case is not quite so strong as he believes. The present mean winter temperature of Bournemouth in lat. $50^{\circ} 43'$ is $37^{\circ} 4'$, but the physical surroundings of Bournemouth are not now such as conduce to luxuriant forest growth, even if its temperature sufficed, and the conditions there in the Eocene time more probably assimilated to those of the south-west coasts of Ireland at the present day.

Now the mean of the coldest month at Valentia, lat. $51^{\circ} 44'$, is 44° , and it may be fairly assumed that if Valentia were a degree farther south, corresponding to Bournemouth, the temperature would be one degree higher; and if sheltered by mountains from all the northerly winds as Glengarriff is, the mean might possibly be raised to 46° . Thus but 11° are required to reach the minimum of 57° supposed to be required by his *Araucaria*. Again, although the Moreton Bay Pine does not appear to support a less mean annual temperature than 67° to 70° between the Clarence and the Bellingen, which are its southern limits in Australia, it flourishes and ripens seeds in Madeira in a mean of 64° to 66° , and although I have only noticed it in two gardens near the sea-level, I think it has only been excluded from others higher up the mountains in favour of the far more striking *Araucaria excelsa*. Moreover from its present restricted area it appears to be a declining type, which may, when more widely distributed, and possibly in presence of fewer competing species in remote Eocene time, have sustained greater extremes of climate.

Taking the species, however, as it exists, and apart from any such possibilities, uniformitarians have, it seems to me, but to account for an increase of 14° to 15° , that is if Bournemouth were near its northern limit, as seems probable from its having grown at or near the sea-level.

Supposing, as all evidence tends to prove, that Northern Europe and America were connected by continuous land in Eocene time, would not the mere fact of shutting off the Arctic Seas cause a general and perhaps sufficient rise of temperature? In N. lat. 70° Prince Albert Land has a mean of only 5° Fahr., and Lapland one of 32° , a difference of no less than 27° , caused solely by the presence of an Arctic ice-laden current. The general cooling effect of incessant oceanic circulation between the North Pole and the Tropics is, I think, scarcely taken into sufficient account, and although it may be contended that conversely the northerly flow of the Gulf Stream mitigates climate, I think that its action in Europe is chiefly in fending off the ice-laden currents from our coasts, the limit of trees penetrating quite as far north in Siberia away from the coast as at the North Cape, where they are under its influence. J. STARKIE GARDNER

Order Zeuglodontia, Owen

IN August 1848 H.M.S. *Dædalus* encountered off St. Helena a marine animal, of which a representation appeared in the *Illustrated News* of the latter part of that year. It is thirty-two years since I saw this figure, but I recollect that it was one of a blunt-nosed animal with a neck carried about four feet above the water, which was so long as to present the appearance of a serpent; and I remember that Prof. Owen, in combating at the time the idea that this was a sea-serpent, pointed out that the position of the gape in relation to the eye, as shown in the figure in the *Illustrated News*, was that of a mammal, and not that of a reptile; in consequence of which he argued that the animal seen was probably only a leonine seal, whose track through the water gave an illusory impression of great length. This idea, however, seemed to me untenable in the face of the representation in the *Illustrated News*; but it was obvious that to afford the buoyancy necessary for the support above the water of so long a neck (estimated on that occasion as sixty feet, though only the part near the head was actually out of the water), the submerged portion of the animal could not have had the shape of a serpent.

Two or three years after this, on reading the description of *Zeuglodon cetoides*, from the Tertiary (probably Upper Eocene) formations of Alabama, it struck me that the animal seen from the *Dædalus* may have been a descendant of the order to which *Zeuglodon* belonged; and I have ever since watched with interest for reports of the "great sea-serpent."

Three years ago the following appeared in the newspapers:—

"Borough of Liverpool, in the County Palatine of Lancaster to wit.

"We the undersigned, captain, officers, and crew of the barque *Pauline* (of London) of Liverpool, in the county of Lancaster, in the United Kingdom of Great Britain and Ireland, do solemnly and sincerely declare that on July 8, 1875, in lat. $50^{\circ} 3' S.$, long. $35^{\circ} W.$, we observed three large sperm-whales, and one of them was gripped round the body with two turns of what appeared to be a huge serpent. The head and tail appeared to have a length beyond the coils of about 30 feet, and its girth 8 or 9 feet. The serpent whirled its victim round and round for about fifteen minutes, and then suddenly dragged the whale to the bottom head first.

"GEORGE DREVAR, Master

"HORATIO THOMPSON

"JOHN HENDERSON LANDELLS

"OWEN BAKER

"WILLIAM LEWARN

"Again, on July 13, a similar serpent was seen about 200 yards off, shooting itself along the surface, head and neck being out of the water several feet. This was seen only by the captain and one ordinary seaman, whose signatures are affixed.

"GEORGE DREVAR, Master."

"A few moments after it was seen elevated some sixty feet perpendicularly in the air by the chief officer and the following able seamen, whose signatures are also affixed—

"HORATIO THOMPSON

"WILLIAM LEWARN

"And we make this solemn declaration, &c.

"Severally declared and subscribed at Liverpool aforesaid, the 10th day of January, 1877, before

"T. S. RAFFLES, J.P. for Liverpool."

The locality here specified was about thirty miles off the northern coast of Brazil.

In this account I thought that I recognised the grip of the whale by the long neck of the attacking animal, the appearance being confounded into the double coil of a serpent by the distance and motion of the objects; but in face of the general ridicule which has been attached to this subject, and being without any assurance that the declaration so purporting to be made was genuine, I did not venture to ventilate my long-cherished idea. A relative of mine, however, just returned from India, chancing to say that two of the officers of the steamer in which she went out had on the previous voyage witnessed an immense animal rear its neck thirty feet out of the water, and that a sketch of the object had been instantly made, and on reaching port sent to the *Graphic*, I obtained the number of that paper for July 19, 1879, and I inclose a tracing of the figures in it, which are accompanied by the following statement in the *Graphic*:—

"The accompanying engraving is a *fac-simile* of a sketch sent to us by Capt. Davison, of the steamship *Kiushiu maru*, and is inserted as a specimen of the curious drawings which are frequently forwarded to us for insertion in the pages of this journal. Capt. Davison's statement, which is countersigned by his chief officer, Mr. McKechnie, is as follows:—'Saturday, April 5, at 11.15 a.m., Cape Satano distant about nine miles, the chief officer and myself observed a whale jump clear out of the sea, about a quarter of a mile away. Shortly after it leaped out again, when I saw that there was something attached to it. Got glasses, and on the next leap distinctly saw something holding on to the belly of the whale. The latter gave one more spring clear of the water, and myself and chief then observed what appeared to be a large creature of the snake species rear itself about thirty feet out of the water. It appeared to be about the thickness of a junk's mast, and after standing about ten seconds in an erect position, it descended into the water, the upper end going first. With my glasses I made out the colour of the beast to resemble that of a pilot fish.'"

As I have not been able to find any description of the skeleton of the *Zeuglodon*, I venture to draw attention to the subject through your columns, in the hope that among your many readers in America this letter may attract the notice of some one who will tell us whether what is known of the osseous structure of *Zeuglodon cetoides* is or is not consistent with the representation in the *Graphic*. The remains of this cetacean, supposed to be extinct, indicate, according to Sir Charles Lyell, that it was at least seventy